Dear Editor,

We include a revised version of our manuscript, will all suggestions of the Referees implemented as detailed below. We hope the manuscript is now acceptable for publication.

Thanks a lot for your efforts and the Referee's work, which we believe has resulted in an improved manuscript

Sincerely,

Ignacio Ribera and Andrey Rudoy

**Editor's Comments**

**MINOR REVISIONS**

As you'll notice from the reviews, both reviewers recognise that the manuscript has improved considerably, but they also indicate a few minor modifications that need to be addressed before the study is acceptable for publication.

**Reviewer 1 (Anonymous)**

*Basic reporting*

As prior to the revision, the manuscript is appropriately embedded in the current literature. The presentation of data has been greatly improved.

*Experimental design*

no comment

*Validity of the findings*

As mentioned earlier, I cannot evaluate the validity of any phylogenetic reconstruction.

*Comments for the Author*

I think that the authors did a good job in revising this manuscript.

The only comment I do have is again related to the "lines of evidence"-argument (I do apologize that I am such a pain).

I agree that the authors do not explicitly sell the results of their different methodologies as "independent lines of evidence". However, reading the lines 344 to 352, the impression that they do, does persists (I believe that the impression of the reader is in this case more important than the intentions of the authors. After all, it is my job to point that out). Although, this may only be an issue of phrasing and maybe not so much a conceptual issue.

Let's have a look at this section (excerpt line 346 to 350):

"(1) the regression between male (y-axis) and female (x-axis) body size had a slope larger than one (i.e. a positive allometry); (2) the ratio male/female body size (rSSD) was correlated mostly with male body size, while the correlation with female body size was lower and in some cases not significant, indicating that males drive the evolution of SSD;..."

In this case, the second finding is given by the first one simply due to the mathematical relationship (and because the same data are used, at least to my reasoning). By stressing both findings equally (in this case by numbering them), and not mentioning that the second analysis is redundant, I get the impression that the same mathematical relationship is used twice to support the same argument. This is clearly not done intentionally but may nevertheless elicit suspicion by certain readers. If this does not concern the authors (which I hope it does), I do not object their decision to leave it as it is.

>> We have revised the phrasing of this section, supressed the numbering and explicitly mentioned that the former second point is a consequence of the first.

**Reviewer 2 (Eduardo Santos)**

*Basic reporting*

There are still some small issues with the language. See my comments below.

The literature is properly cited and used, and the data is now shared as supplemental files.

*Experimental design*

The authors addressed comments that both myself and Reviewer #1 made about the research question that helped to make the interpretation of the study's design easier. The methods are now sufficiently described and more clear than in the original version of the manuscript.

*Validity of the findings*

With the changes made to the manuscript, I am now comfortable with the validity of the findings and the conclusions provided by the authors.

*Comments for the Author*

Comments on MS #14209v2

L53-56: I had previously inquired about the use of the term “population fertility” (see my original comment L47-53), to which you replied that you had removed this sentence from the revised version of the manuscript. However, upon reading the revised version, I found that the sentence is still there, with a slight change in how the term is used. I still believe that the use of the term “fertility of a population” is equivocal. I think that you can remove the passage “the fertility of the population depends more on females than on males,” and use only the rest of the sentence to make your argument about the evolution of body size in males and females.

>> We have deleted the part of the sentence referring to "fertility of a population".

L67-69: Thank you for the explanation about what you meant by “social reason”. It is now clear to the reader.

L72: There is a comma after “body, but also other…”. Moreover, change “used for male-male” to “used in male-male”.

>> Done

L73-75: Rewrite as “Similarly, in the latter case, in addition to male body size, other characters may be involved, especially genital characters…”

>> Done

L78: Change “a number of” with “several”.

>> Done

L94-97: Thank you for addressing this bit, it is much clearer now.

L103-106: Thank you for clarifying this bit.

L112: “In this work, …”

L134: “As a study group, …”

L153-165: Thank you for rewriting this section, it reads much better now.

L191: “In most analyses, …”

L192: Ditto

L215-219: In your response letter, you explained very well what the fractal dimension represents biologically. However, you did not change the text in this method section. I would strongly recommend that the authors change the text here to make it clearer to readers that are not familiar with the approach. It is not enough to ask readers to refer to Rudoy et al. 2016 for further details. Most readers will not do this and at this point you have lost the interest of your audience.

>> We have included an additional explanation on the biological meaning of the fractal dimension, basically copied from our first response letter.

L231: “For our analyses, …”

L264: “Due to these limitations, …”

>> Done.

L266: “secondary loss”

>> Corrected.

Other than these comments, I am happy with the changes that the authors have made to the manuscript and believe that it will be an interesting contribution to the field of the evolution of sexual size dimorphism.